The immediate source of attraction possessed by the flower for its feathered visitants lies, I think, in the small insects which resort to it, and not, at any rate usually, in their furnishing any nectareous secretion which is palatable to the birds. For if the latter visited the blossoms for the sake of the nectar they would be perfectly acquainted by experience with its situation and make no delay in going straight to it, whereas the habit of the sun-birds and the flower-peckers also is rather to hover on rapidly-vibrating wings a few inches in front of the opening of a blossom, as if prying into its recesses in search of food, before thrusting their beaks into the corolla; and often after thus examining a flower they fly off to another without touching it at all, having apparently satisfied themselves that the first one contained no prey for them.

A. H. EVERETT

N. Mindanao, July 23

Heat Phenomena and Muscular Action

ON reading the article which appeared in NATURE, vol. xvi. p. 451, on the heat phenomena accompanying muscular action, it has occurred to me to send the following problem which is akin to the subject.

If a man does work (say lifts a weight), the principle of the conservation of energy teaches us that the potential energy—the work done—(weight lifted) is at the expense of the man as a magazine of force, in fact that "virtue has gone out of him." Now suppose a man lifts say a ton of bricks and deposits the bricks one by one on the top of a wall six feet high, we can exactly estimate the amount of work done, the energy rendered potential and external, and if we knew also the extra amount of heat radiated or otherwise carried off from his body—as most probably the work would raise his temperature—we could exactly measure the amount of energy the lifting of the brick cost him.

Now suppose another man were to lift the bricks from the top of the wall and deposit them gently—*i.e.*, without concussion—on the ground, it is evident that there is a certain amount of potential energy disappearing, in fact that there is work being absorbed by the man, of course appearing in some other form, but the question is how? This second man's work is of course in one sense work, but in the sense of producing external, potential, or kinetic energy, is not so, unless, perhaps, in heat.

Strangely enough it follows that lifting down the brick ought to make the man either radiate heat more, waste tissue less, digest food less, or in some other way account for the energy absorbed by him.

Generally I think the conversion of force by obstruction is not always so clearly traced as it might be; in friction it is clear, as also in the compression of elastic bodies, but in the instance above, as also in the throttling of steam, it is not so clear.

A. R. MOLISON

Does Sunshine Extinguish Fire?

I READ Mr. Tomlinson's paper (NATURE, vol. xvi. p. 361) near the time of its delivery, and was struck with the inconclusive character of his experiments. What he attempted to obtain was the condition of combustion in sunshine and combustion in darkness, *cæteris paribus*. But he left the *cæteris paribus* entirely out of the experiment, and actually used a dark cubbard (I believe this is good spelling etymologically and phonetically), into which there was no free influx of atmospheric air. Naturally his candles burnt with inferior combustion there. I have for years together burnt Newcastle coal, and no other; and for years together burnt South Staffordshire coal, and no other; and I say that sunshine puts out a sea-coal fire and not a S.S. fire. The reason of this is, I apprehend, not far to seek. In the Midlands it is the practice to keep a fire alive by a raker, or gathercoal. It would be quite useless to attempt to do this with a sea-coal fire, which goes out in a short time unless the cakes of coal be broken up; in a word, one has to watch a sea-coal fire; and it must be in every Londoner's experience, that such a fire is apt to elude one at the last faint gleam from over reckless poking. Now, if the sun is shining on the coal, that last faint gleam is invisible, and the fire goes out as a matter of course. Sunshine puts out a sea-coal fire by insidiously eclipsing the warning glimmer of its expiring embers. This, at least, is a *vera causa*. *A priori* I should say that combustion would be less rapid in air rarefied by sunlight than in air deprived of it; but I do not believe sunshine extinguishes a coal fire in any other way than that I have described.

C. M. INGLEBY

Folkestone

OUR ASTRONOMICAL COLUMN

THE APPROACHING OPPOSITION OF IRIS.—The opposition of this minor planet in the present autumn affords another favourable opportunity of determining the amount of solar parallax on the method already successfully applied by Prof. Galle, of Breslau, in the case of Flora. The *Berliner astronomisches Jahrbuch* for 1879 contains a rough ephemeris of Iris for every twentieth day of the year, but this being insufficient for the purpose in view, we subjoin places calculated from Prof. Brünnow's tables of the planet, on the approximate formulæ explained in his introduction; the error of the tables being very sensible at the present time, nothing would have been gained by calculating in the accurate form. For the sake of brevity the planet's positions are given for every fourth day only, but they will be readily interpolated for the intermediate dates.

IRIS.—*At Greenwich Midnight.*

		Right Ascension. h. m. s.	North Declination. ° ′	Distance from the Earth.	Distance from the Sun.
Oct.	8	3 56 7	27 5·6	1·0034	1·8350
,,	12	3 56 50	27 3·3	0·9769	
,,	16	3 56 55	26 57·8	0·9526	1·8345
,,	20	3 56 20	26 48·9	0·9306	
,,	24	3 55 8	26 36·5	0·9111	1·8354
,,	28	3 53 20	26 20·5	0·8944	
Nov.	1	3 50 58	26 0·9	0·8808	1·8376
,,	5	3 48 8	25 37·8	0·8704	
,,	9	3 44 57	25 11·6	0·8633	1·8411
,,	13	3 41 32	24 42·7	0·8597	
,,	17	3 38 1	24 11·5	0·8597	1·8459
,,	21	3 34 31	23 38·7	0·8634	
,,	25	3 31 9	23 5·0	0·8708	1·8520
,,	29	3 28 2	22 31·2	0·8819	
Dec.	3	3 25 17	21 58·1	0·8965	1·8593
,,	7	3 22 58	21 26·5	0·9146	
,,	11	3 21 11	20 56·7	0·9360	1·8677

Iris will be in perihelion October 14·7, G.M.T., and nearest to the earth on November 15, her distance at this time being 0·859 (the earth's mean distance from the sun being taken as unity). Her intensity of light may be expected to rather exceed that of a star of the seventh magnitude, 6·8m. according to the *Berliner Jahrbuch.*

THE OUTER SATELLITE OF MARS.—This object is still under observation at the Observatory of Paris. It was also measured again by Mr. Common, of Ealing, with his 18-inch silver-on-glass reflector on September 24, the angle calculated from the elements which have been given in this column differing from the observed angle $-4°$. An observation on September 13, by M. Borrelly at Marseilles, presumed to apply to the satellite, must refer to a faint star, the satellite at the time being in the opposite quadrant.

BINARY STARS.—Dr. Doberck, of Markree Observatory, continues his investigations on the orbits of the revolving double stars. In No. 2,156 of the *Astronomische Nachrichten* he has given provisional elements of Σ 1768 and Σ 3121, the latter of which appears to be an object of special interest from the shortness of the period of revolution, which hardly exceeds that of the well-known binary, ζ Herculis. Also elements of Σ 3062, a star which was the subject of a pretty complete calculation by Dr. Schur in 1867. The results of the two discussions are as follow:—

	Schur 1867	Doberck 1877
Passage of peri-astre	1835·196	1834·88
Node	32° 10′	38° 35′
Angle between the lines of nodes and apsides	97° 31′	92° 7′
Inclination	29° 58′	32° 11′
Eccentricity	0·5009	0·4612
Semi-axis major	1″·310	1″·270
Period of revolution	112·64 years	104·415 years

These orbits are in very satisfactory confirmation of each other.

May we hope that at no distant period Dr. Doberck may find he has sufficient material to induce him to investigate the elements of α Centauri; a fair approximation to the true orbit might be expected from his experienced hand.

PROF. ADAMS ON LEVERRIER'S PLANETARY THEORIES[1]

II.

THE nineteenth chapter of M. Leverrier's researches, which forms the first part of the eleventh volume of the *Annals of the Paris Observatory*, contains the determination of the secular variations of the elements of the orbits of the four planets, Jupiter, Saturn, Uranus, and Neptune.

In the first place are collected the differential formulæ which are established in the previous chapter, and which give the rates of secular change of the various elements at any epoch in terms of the elements themselves, which by the previous operations have been cleared of all periodic inequalities.

The terms of different orders which enter into these formulæ are carefully distinguished.

If we were to confine our attention to the terms of the first degree with respect to the excentricities and inclinations of the orbits, and of the first order with respect to the masses, the differential equations which determine the secular variations would become linear, and their general integrals might be found, so as to give the values of the several elements for an indefinite period.

In the present case, however, the terms of higher orders are far too important to be neglected, and when these are taken into account the equations become so complicated as to render it hopeless to attempt to determine their general integrals.

Fortunately, however, these are not needed for the actual requirements of astronomy, and for any definite period the simultaneous integrals may be determined with any degree of accuracy that may be desired by the method of quadratures.

In this way M. Leverrier has determined the values of the elements for a period of 2,000 years, starting from 1850, at successive intervals of 500 years. The first steps in this integration were attended with some difficulties, because the determination of the numerical values of the rates of change of the several elements at the various epochs depends on the elements themselves which are to be determined. Hence several approximations were necessary in order to obtain the requisite precision.

After this work of M. Leverrier, however, the extension of the investigation to other epochs, past or future, is no longer attended with the same difficulties. In fact, from his results we may at once find, by the method of differences, very approximate values of the elements at an epoch 500 years earlier or later than those which he has considered. His general formulæ will then give the rates of change of the several elements at the epoch in question, and having these we can determine by a direct calculation the small corrections which should be applied to the approximate values of the elements first found.

This process may evidently be repeated as often as we choose.

It is important to remark that in the formulæ which give the rates of change of each of the elements at the five principal epochs considered, as well as in those which give the total variations of the elements at the same epochs, the masses of the several planets appear in an indeterminate form, so that it may be at once seen what part of the variation of any element is due to the action of each of the planets, and what changes would be produced in the value of any element at any epoch by any changes in the assumed values of the masses.

Consequently, when the astronomer of the future, say of 2,000 years hence, has determined the values of the elements of the planetary orbits corresponding to that epoch, it will be easy for him, by comparing those values with the general expressions given by M. Leverrier, to determine with the greatest precision the actual values of the masses, provided that all the disturbing bodies are known; and should there be any unknown disturbing causes, their existence would be indicated by the inconsistency of the values of the masses which would be found from the different equations of condition.

By means of the work which has just been described, everything has been prepared which is required for the treatment of the theories of the several planets.

The remainder of the eleventh volume of the *Annals* is accordingly occupied by the complete theories of Jupiter and Saturn, the former theory being given in Chapter 20, and the latter in Chapter 21 of M. Leverrier's researches.

The coefficients of the periodic inequalities of the mean longitudes and of the elements of the orbits are not only exhibited in a general form, but are also calculated numerically for the five principal epochs considered in Chapter 19 of these researches, viz., for 1850, 2350, 2850, 3350, and 3850.

The long inequalities of the second order with respect to the masses, depending on twice the mean motion of Jupiter plus three times the mean motion of Uranus minus six times the mean motion of Saturn, are also determined in a similar form.

Chapter 22 of M. Leverrier's researches, forming the first part of the 12th volume of the "Annals," contains the comparison of the theory of Jupiter with the observations, the deduction of the definitive corrections of the elements therefrom, and finally the resulting tables of the motion of Jupiter. The observations employed are the Greenwich observations from 1750 to 1830 and from 1836 to 1869 together with the Paris observations from 1837 to 1867.

To the results given in the Astronomer-Royal's "Reduction of the Greenwich Observations of Planets from 1750 to 1830," M. Leverrier has applied the corrections which he has found to be required by his own reduction of Bradley's observations of stars and his redetermination of the Right Ascensions of the fundamental stars, published in the second volume of the "Annals" (Chapter 10).

The equations of condition in longitude, for finding the corrections of the elements and of the assumed mass of Saturn, are divided into two series corresponding to the observations made from 1750 to 1830, and into two other series corresponding to the observations made from 1836 to 1869. Moreover in each of these series the equations are subdivided into eight groups, corresponding to the distances of the planet from its perihelion, 0° to 45°, 45° to 90°, and so on. From these are formed four final equations, the solution of which gives the corrections of the epoch, of the mean motion, of the excentricity, and of the longitude of the perihelion, in terms of the correction required by the mass of Saturn, which is left in an indeterminate form. The substitution of these expressions in the thirty-two normal equations corresponding to the several groups above-mentioned, gives the residual differences between theory and observation in terms of the correction of the mass of Saturn. No conclusion can be drawn from the ancient observations; but from the modern observations M. Leverrier finds that the mass of Saturn assumed—which is that of Bouvard—should be diminished by about its $\frac{1}{200}$th part. This correction is very small, but M. Leverrier regards it as well established.

On the other hand, Bessel's value of the mass of Saturn, founded on his observations of the Huyghenian satellite, exceeds Bouvard's by about its $\frac{1}{350}$th part.

[1] Continued from p. 464.